Earth Syst. Dynam. Discuss., 1, C211–C215, 2011
www.earth-syst-dynam-discuss.net/1/C211/2011/

Earth System Dynamics Discussions

Interactive comment on "The problem of the second wind turbine – a note on a common but flawed wind power estimation method" *by* F. Gans et al.

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Received and published: 27 May 2011

We thank K Hasselmann for his time and his review. The intention of this manuscript was not to give a quantitative estimate of the extractable wind power in the boundary layer. The title as well as the introduction make it clear that the paper focuses on explaining the differences in the results of 2 wind power estimation methods. It was not the purpose of the paper to arrive at an improved estimate for wind power extractability, and nowhere in the paper it is claimed that we do so. Instead, the paper contributes to the scientific process by demonstrating why the results in estimating large-scale wind power extractability as well as the associated impacts differ significantly. This paper

also demonstrates why accounting for the removal of free energy from the atmosphere is essential. In response to the reviewer's comment on:

> ***1)*** *"It is not possible to deduce the total amount of energy that can be extracted from the atmosphere through wind farms alone from a "tunnel view" of the boundary layer. Important is how the boundary layer is coupled to the total kinetic energy of the atmospheric circulation, and how fast, in turn, the extraction of kinetic energy from the total atmospheric circulation through wind farms can be replaced by energy from the sun's radiation."*

We fully agree. As already said, this was not the original intention of our manuscript.

> ***2)*** *"In other words: the problem can be tackled quantitatively only with an atmospheric general circulation model. It should not be too difficult to modify an AGCM by introducing a higher friction at the surface, or, more precisely, through a higher (tower-like) resistance in the boundary layer which reproduces the desired effect. I wonder why the authors have not referred at all to the work of Wang and Prinn who have apparently already done something like that."*

We agree with the reviewer and understand his concerns about our very simplified setup here. The same authors of this manuscript have also completed the analysis the reviewer is suggesting (Miller2011), and the results are similar to the one obtained by the simple setup. In response to the reviewers comment, we have added some of the GCM results to this manuscript further illustrating that the simplified setup still captures the most important dynamics.

We are also well aware of the mentioned work by Wang and Prinn (Wang & Prinn (2010)) - a compiling error during submission was overlooked by the authors. We apologize to the reviewer and Wang & Prinn for this shortcoming.

> ***3)*** *"The errors of other authors (Magdalena & Jacobson, and Archer & Caldeira) in estimating the maximal possible energy abstraction are pointed out. These are due apparently to wrong assumptions of the authors regarding the kinetic energy of the lowest 1 km of the atmosphere and the jet stream, respectively.- accepting the numbers given by Gans et al. This criticism is perhaps correct, but in no way justifies the authors' own calculations."*

The main intention of the manuscript was to identify and clarify these errors made by other authors utilizing the 'common method.' We appreciate that the reviewer agrees on this. We are alarmed by the reviewer's suggestion that our own calculations are not justifiable and would be happy to correct them but they were not explicitly identified. We have reviewed our calculations again but are unable to identify the calculation error or a wrongful-conclusion that is derived from the calculation.

> ***4)*** *"Acceping the numbers given by Ganz et al (I did not check them), namely that the total production of kinetic energy of the atmosphere is about 900 TW while the human use is about 17 TW, i.e. about 2 percent of this amount, the estimate of Wang and Prinn (cf.. anonymous reviewer no 2) appears plausible, namely that human energy extraction would reduce the kinetic energy of the boundary layer by about 10 to 20 percent, if the extraction was applied worldwide to continental boundary layers. Conclusion: The paper should be judged in relation to the work of Wang and Prinn. In contrast to Wang and Prinn, the authors offer no numbers, only the general*

> *statement "significant reduction". If they can improve on Wang and Prinn's estimate, fine. But a tunnel model won't do it. "*

The subject of this manuscript was not to give an exact estimate of this impact since this is done in other articles (Wang & Prinn (2010); Miller et al. (2011)), but in response to the reviewers comment we have included some GCM results in the revised version of our manuscript. However, the aim of our study was not to improve the estimates of these studies on extractable power or climatic impacts. Since the results presented in (Wang & Prinn (2010); Miller et al. (2011)) significantly differ from other studies using the common method, we feel there is a need to identify the reason for this discrepancy.

References

Wang, C., & Prinn, R. G. (2010). Potential climatic impacts and reliability of very large-scale wind farms. Atmospheric Chemistry and Physics, 10(4), 2053-2061. doi: 10.5194/acp-10-2053-2010.

Miller, L. M., Gans, F., & Kleidon, A. (2011). Estimating maximum global land surface wind power extractability and associated climatic consequences. Earth System Dynamics, 2(1), 1-12. doi: 10.5194/esd-2-1-2011.

Interactive comment on Earth Syst. Dynam. Discuss., 1, 103, 2010.

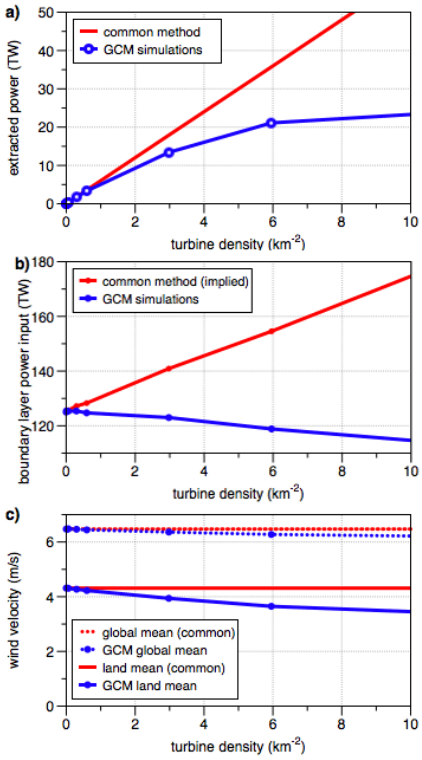

Fig. 1. Mean power extraction, mean boundary layer dissipation, mean global wind velocity and mean land wind velocities are shown. The estimates of the common methodology are depicted by red lines and modeled